SOME FUNDAMENTAL CONCEPTS IN TAXONOMY

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Since the beginning of taxonomy as a science, which we may date at 1753, it has been beset by objectors. Every innovation in the accepted procedure has been criticized. Linnaeus' sexual system, the first forward step in classification in half a century, had its opponents. Binomial nomenclature was not fully adopted for a few decades. Except binomial names, which had long been and still are in common use by various peoples, and except the subordination of species to genus, which dates back at least two thousand years, every important principle and practive in taxonomy has been an innovation, and we may be sure that each of them has received its due share of objection and criticism before it was generally accepted. Even these may theoretically be discarded in the future, just as various other ideas have already been permanently rejected after experience with them.

The reason for this condition will be obvious, if one will stop to reflect on the fundamental nature of taxonomic research. Our science is not an exact mental discipline, as mathematics; neither is it an exact experimental science, as chemistry or physics. No field of botany can compare with these sciences in precision; genetics and plant physiology offer the nearest approach to them.

Taxonomy must be regarded as an interpretative science. We observe various phenomena of plants as completely and as accurately as we can with our limited powers. We cogitate on our observations and the result of our thought is expressed as a taxonomic conclusion. We have interpreted our observations and formed an opinion.

Those who engage in taxonomic research have always previouly acquired a considerable body of botanical knowledge. This knowledge is unavoidably a potent factor in directing and controlling our taxonomic conclusions. We invariably try to make our opinions conform to ous established ideas. Usually we succeed, and we emerge with an opinion which offers nothing new but is merely an extension or continuation of the prevailing thought of the time. Innovations in taxonomic thought and procedure, some of which fall, some of which are eventually generally adopted, come only from those with the courage to depart from convention, a courage which is forced upon them by the weight of their own observations.

Taxonomists no longer propound theories which are based on pure speculation without some foundation of observable fact.

They may carry their reasoning further than their basic knowledge justifies, or we may think that they do if we disagree with them, but back of every theory there is always some fact. Botanical facts enter our mind through our five senses, chiefly through our sight. Taxonomists have, in general, about the same powers of observation. What is known to one is known to another, or can be verified by another if he desires. To be sure, no one knows everything about any plant, and new facts are constantly being discovered and our existing ideas amended accordingly.

After taking full account of new discoveries, after making every allowance for errors in our conclusions caused by incomplete or faulty observation, it is still apparent that the mental processes of taxonomists, by which these facts are digested, differ so widely and have differed so continuously that various taxonomic matters have been in controversy for two centuries. If we look over the field of taxonomy even hastily, we can find three general subjects on which there has been, is, and probably long will be great discussion. In two hundred years we have reached no permanent conclusion.

Probably most of us will think first of the problems of nomenclature. The International Rules, since their adoption in 1867, have been considerably amended three times and somewhat changed twice. Scores of proposed amendments, often conflicting, submitted to the Stockholm Congress by scores of taxonomists from many countries, indicate the dissatisfaction and disagreement still prevalent among botanists. If the experience of the past five sessions of the International Congress can be any guide to the future, we may confidently expect still other proposed changes and still other adopted amendments at the next meetings. Nomenclature, however, is not a taxonomic problem. It has nothing to do with the kinds of plants and deals only with the names applied to them. We exclude it from further discussion here.

A second problem is the arrangement of the families and orders in accordance with their probable phylogeny. The change from the old sequence of Bentham and Hooker to that of Engler and Prantl was severe, but no greater than the change from both advocated by Hallier and in this country by Bessey, Schaffner, and Pool, or the still later proposals of Lotsy, Hutchinson, Pulle, and others. Although phylogeny is one of the most important problems of taxonomy, it will not be discussed further here.

Distinctly taxonomic is the third problem of the nature and scope of the species, the genus, and the various other categories of classification. Much has been written on the concept of the genus, on the difference subspecies and variety, and similar topics. Many attempts have been made to

define the species, and most of the definitions have been fully satisfactory only to the definer. This is the problem which we propose to discuss in the following paragraphs. If the reader expects to find a weighty pronouncement on any of these matters all ready for him to demolish by his own superior wisdom, he would do better to turn to the statements of various contemporaries who apparently know all about them. If he expects to chortle in glee at the wit and sarcasm which we shall direct against some hapless botanist, he had better lay this down and pick up a copy of the New Yorker.

The concept of genus and species is not a new one, although some biosystematists give one the impression that they are the original discoverers, or even the inventors of the idea. The concept is vastly old, and because it is so old we have to consider some of the fundamental ideas which underly it. Of course it has changed somewhat with the increase of botanical knowledge, and its use by the taxonomist of today differs somewhat from its earlier use by the laity. Just the same, its present connotation in even our most advanced (or most radical) taxonomic work is a direct result of its earlier and simpler history. Our science has grown, both in breadth and depth, out of the contacts of early man with the world of plants about him. The changes which we may detect are the changes of a normal ontogeny. As the poet said, "the child is father to the man."

The concept of species and genus is inextricably bound with certain matters of semantics and certain mental processes. To get it properly before us, I propose to discuss three general concepts or principles upon which taxonomy is founded and a fourth which we have developed for ourselves. Some of these will have to be presented in a very elementary way.

As every taxonomist knows, the original meaning of genus in the Latin tongue was a kind of something, while a species was a special kind. From the general meaning of the two words in ancient times, through the loose application to plants by the pre-Linnaean fathers of botany and their restricted application in post-Darwinian time, down to the present day, the words have changed from the common speech of the laity and become technical terms of the taxonomist. Nevertheless, the words still carry inherently within them some implications derived with little or no change from the original usage. The most apparent of these is the subordination of species to genus. There is nothing in the origin of the words to indicate that every species must belong to a genus or that every genus must contain a species. The next apparent implication in meaning is more obvious in the English translation kind. As soon as we stop to consider, we realize

that in every instance, we know or we infer from past experience that a kind of plant includes a number of individuals all more or less alike. Washington Elm or Charter Oak or General Grant Sequoia do not denote kinds of plants; they are proper nouns applied to individuals. Ulmus americana or American Elm denote a plurality of individuals! they are common nouns. The mere fact that each of these terms consists of two epithets does not affect their standing as nouns, even though the grammarian may insist that half of each term is adjectival. This brings us to the first and most elementary idea in taxonomy, the population.

The Concept of Population

This word, derived from the Latin populus, people, soon lost its original meaning in English and became, according to Webster, "the whole number of people or inhabitants in a country, section, or area." It was only a short step from this meaning to apply it to other organisms besides Homo sapiens. It was taken over by the biologist and used to indicate, again quoting Webster, "the organisms, collectively, inhabiting an area or region." Such an expression as 'the bird population of an orchard' is intelligible to anyone. It is equally practicable to speak of the 'weed population' of the same orchard. Still later, according to Webster, the word was used in statistics to denote "the entire group of organisms, from which samples are taken for measurement," or any "group of persons, objects, or items." In this sense the word is commonly used in various branches of science today.

How does it happen that a group of objects can be established? What is the real basis of this concept of population? Webster's definition of the cognate word people emphasizes a point which also appears but is not clearly expressed in the other definitions. People denotes a body of persons "united by a common character," and it is at once apparent that the only basis and the only reason for the modern concept of population lies in the similarity of the component individuals. As used by the biologist, the word is always a collective noun and connotes a plurality of individuals all of which have some point or points of similarity. This likeness may be of any kind qualitatively (that is, based on any common character) and of any degree quantitatively. Thus, the bird population of an orchard is composed of individuals with just two effective points of similarity: they are all birds and they all live in the same orchard. From this loose but perfectly valid use of the term, there is every gradation to the highly restricted populations of the geneticist, who may have for study a group of plants or

animals in which the individuals are so closely related by descent, so closely similar in every observable character, that they are essentially identical.

When population was defined as a group from which samples were taken for statistical study, it might more appropriately have been stated 'for any kind of scientific study.' With either term, the selection of samples and the conclusions drawn from their study are based on the assumption that all the individuals of the population are alike, at least in the features which are the subjects of the study, and therefore that the results derived from the study of some will be applicable to the group as a whole.

Now that is precisely what we do in taxonomy. Our studies of a kind of plant are never based on the whole population, but only on that fraction of the total which we are able to see alive in the field and preserved in the herbarium. When we write a description of a species or a genus or any other taxonomic group, we believe that our description will apply to all the individuals which constitute that unit.

It is only natural that the new systematists' should adopt the term population for groups of individuals which have some sort of taxonomic status. They find it to be a very useful term and other taxonomists will also use it to advantage.

Ecologists think without mental strain of the bird population of an orchard, a group whose only taxonomic similarity is their membership in the class Aves, or even of the biota of the same orchard, including all the plants and animals. Taxonomists and geneticists commonly use the term in a more restricted sense, limiting it to a species or part of a species. It is equally applicable to any superior group. A genus or a family is composed of a number (presumably large) of individuals all possessing in common a certain set of characters. The use of the term population for such a group may seldom be useful, but it is certainly technically admissible.

The Concept of Kind

In taxonomy, the first and most fundamental of all concepts is that of the existence of kinds of plants. Kinds were recognized long before the emergence of botany as a science. Dioscorides and Theophrastus and Vergil wrote about them; scores of kinds are named in the Bible. Even the first chapter of Genesis says "Let the earth bring forth the living creature after his kind." They were known before language was committed to writing; all contemporary primitive races know them and have names for them. They antedate the human race; certainly the monkeys distinguish kinds, eating the one and refusing the other. They were distinguished at a still

level of animal evolution; the Zebra Swallowtail butterfly flits through our woods and deposits its eggs only on the Papaw.

The only way to understand the basic nature of the concept, to formulate an adequate definition of kind, is to consider many kinds of plants and ascertain what characters they possess in common. Since we desire to get to the fundamentals of the problem, we must draw our ideas not from the kinds of plants accepted by the modern taxonomist, but from those known to the layman. I have developed the ideas which are presented below not from literature or from the opinions of professional taxonomists, but from actual conversation with a number of persons of intelligence but uneducated and untrained in botanical matters.

First, a kind consists of and includes a plurality of individuals. So far as the actual number is concerned, it may vary from the score of Sequoias which he sees on a vacation in California to the millions of plants of wheat which he raises on his Kansas farm. The taxonomist will retort that there are many kinds of plants known only through a single individual. That is true, but, in originally describing a species from a single individual, the taxonomist tacitly assumes or piously hopes that additional plants will eventually be found and experience teaches that they usually are. It is of course theoretically possible that a kind might arise by evolution as a single first plant, and practically possible that a moribund kind might persist as a single last individual, as did that last passenger pigeon in the Cincinnati zoo.

Second, the individuals of a kind are alike, perceptibly alike to the human senses. Keeping to the kinds known to the layman, we can easily see that the likeness may be of any degree and lie in any or all features of the plant. We observe, in general, that striking or conspicuous features or superficialities are sometimes given greater weight than by the taxonomist. An orchardist, for example, will speak of the many different kinds of apple, basing his kinds on the color, size, shape, and flavor of the fruit, while the taxonomist is content with the single kind Pyrus Malus. The layman very seldom tries to analyze what the features of similarity are. He recognizes a kind by the totality of its characters and is often unable to tell or lacks the words to tell how or why he knows it. In the field, the taxonomist recognizes plants in exactly the same way. It is only in writing for others that he finds it necessary to express in words certain distinctive and often minute diagnostic characters.

Third, a kind of plant is not divided. It seems to be an indivisible unit. If a group of plants can be and is divided

by the layman, the result is not subdivisions of a kind but two or more kinds. I once got a number of ideas from a farmer that I knew. I asked him the name of a plant growing along his fence, I believe it was Specularia perfoliata, and his reply was "That's just a weed." I wondered if I could now get any evidence that to him a 'weed' was one kind of plant. So I asked him about another, and he replied that it was "another kind of weed." In other words, among observant people, such as a farmer with his weeds, a lumberman with his trees, a gardener with his flowers, kinds are recognized but names are not always available to apply to them.

Fourth, every kind of plant, so far as known to the layman by actual experience, has genetical continuity, and this belief is instinctively extended to every other kind of plant as well. It is axiomatic in the minds of all people who have any contact with plants. It dates back to the very dawn of agriculture. Nineteen hundred years ago it was so well established that the Apostle wrote to the Galatians "Whatsoever a man soweth, that shall he also reap," and on this platitude as a text he developed a sermon. To be sure, the layman has not demanded continuity through seed-reproduction; he has long known that some kinds of plants will not 'come true' by seed and must be maintained by grafts or cuttings.

The recognition of kinds of plants, each one with morphological similarity and genetical continuity, is fundamental to all our practical work in agriculture, horticulture, and forestry and to all our research in pure botany. The cytologist who observes the various stages in the development of the embryo-sac of Lilium Martagon correctly concludes that the same phenomena occur in every other plant of the same kind. The physiologist who finds that seeds from a certain individual plant will not germinate until they have been frozen correctly concludes that seeds from any other plant of the same kind will need the same treatment. And in general, all our botanical discoveries are made from individuals and are then extended to cover all plants of that particular kind.

This analysis of kind has been drawn from contemporary experience. There is no reason to think or to expect that the concept among thelaity has changed in the last several centuries; in the writings of herbalists and pre-Linnaean botanists there is no evidence that it has changed. Kinds were accepted intuitively and no one stopped to ask or consider whether a certain population should be called a kind or not. The characters of a kind, which I have tried to reduce to four, were axiomatic. Every one knew that there were many pea vinesin the world; that they were all so much alike that one name would serve for all of them; that they were still

peas, even though they did vary a little in size or other features; and that ripe peas, if planted, would produce another crop of peas next year. Nothing could be simpler.

And yet these kinds (excepting horticultural varieties, as we now know them), called that or some other homely term by the English writers, or some equally familiar term in the other languagesof the time, were known by the equivalent Latin word species when they were discussed by the learned. As species they came down to Linnaeus and were accepted by him; as species we still know them today.

If we next compare the modern popular concept of kind with the modern taxonomist's concept of species, we find that they are still essentially identical. We admit freely that there are kinds accepted by the laity which will not be regarded as species by the taxonomist. Some of these are based on too narrow a degree of similarity, as the varieties of apples. Some lack the sort of genetical continuity which the taxonomist usually requires, as the varieties of cultivated Dahlia, although I never knew a taxonomist who rejected Lilium tigrinum as a species because it produces no seeds. Some species are distinguished by characters which the layman can not or does not easily observe and are accordingly neglected by him; these are usually accepted by the layman as soon as their characters are pointed out. (I remember the intelligent man in Illinois who knew just one kind of Milkweed, not distinguishing between Asclepias syriaca and A. Sullivantii, and I remember the smile that spread over his face when I showed him the difference between them.)

The accuracy with which the layman knows kinds in precisely the same sense as the taxonomist knows his species is often surprising. I have remarked on this matter before, but will repeat some instances. A boy of fourteen in California, who had not studied even high school botany, told me there were four kinds of Filaria. I identified them to four species of Erodium as accepted by Jepson in his Manual. A Maryland farmer was asked about a tree which grew near his home. He relied "That is a 'specie' of Red Oak." So it was: the Southern Red Oak or Spanish Oak, Quercus falcata. A botanist at Buitenzorg told me that the Malays who served as field assistants in botanical exploration had names for almost all kinds of plants and that their use of a different name was an almost infallible guide to a different species, even though its specific characters were not immediately detected by the botanist. I had personally the same experience with an Indian in South America, but soon discovered that there were certain groups of plants, notably the epiphytes, for which he had no specific names. It may seem a blow to our taxonomic pride, but we might conclude that our own discernment of species

depends on the same sort of intuitive recognition possessed by all people in all countries and at all times. We merely go a step farther, analyze the cause of our intuitions, and reduce our findings to words.

Kind and Population

In now becomes necessary to consider in more detail the relation between these two concepts. Since both denote groups of individuals and are characterized by similarities among these individuals, it is clear that a kind of plant is a population. It should be equally clear that not all populations are kinds. Some groups lack the morphological similarity or the genetical continuity requisite to the concept of kind.

I wish to develop this thesis by using one particular kind of plant as an example. It is our common source of hay fever, known to us as Ambrosia trifida and to the layman as Ragweed or Horseweed. It constitutes a population, unified by the possession of structural features. Because of them you and I know the plant at sight, even from a considerable distance. Most plants have three-lobed leaves; some have five-lobed; in every community some may be found with ovate unlobed leaves; still others have part of the leaves lobed and part unlobed. Some plants have a single terminal spike of flowers; others branch above and bear several spikes. Some plants are tall and bear many pairs of leaves; others are short and bear only a few pairs. All combinations of these three sets and of still other characters may be made, and every such combination can be illustrated by a group of individuals. Each such group will fully correspond to our idea of a population. Every set of features, as chosen by me, is distinctly morphological in nature. On just three classes of characters, lobing of leaves, number of spikes, and number of nodes, one might easily distinguish a hundred populations, every one fully definable. A similarly large number of populations may be differentiated within any species by any one who cares to waste his time at it. Some taxonomists have.

"Balderdash," says the taxonomist, an expression which might well be couched in shorter and uglier words. But I have made no claims that these populations have any importance. They are merely to illustrate the fact that in any species an indefinitely large number of populations can be distinguished.

Now that each of you taxonomists who may read this paper has turned from these minor populations in Ambrosia trifida with abhorrence or disgust, let me ask you frankly why. Your

first reply will probably be that they are not worth bothering with. I admit that, but it is not a real answer. We want to know why they are not worth bothering with, especially since they are all based on morphological characters. Analyze your own ratiocination carefully and you will soon get the answer. Basing your opinion on your experience with these plants and with many others, you believe that these plants do not have genetical continuity. They are not races in which the distinguishing characters are transmissible to the progeny. You do not know that; you merely infer it, and you are almost certainly right. So there we are, right back to one of the pre-Linnaean concepts of kind rife for hundreds of years among the laity, that a kind is a self-perpetuating population. You will also say that the nature (qualitative) and the degree (quantitative) of the differences between these populations is not sufficient to warrant taxonomic recognition. But notice that this conclusion is not a matter of botanical fact but merely one of personal opinion. Nevertheless, it brings us back to another fundamental concept of kind, that a kind is the smallest population which is not divided. A kind can be divided but is not, because the differences separating the minor populations are trivial when measured by our standards.

Consider two other examples. The cultivated Dahlia is now separable into several thousand populations which have received names from the horticulturists. The differences which separate them are so great, according to his standards, that they need names; to him they are kinds of plants. In fact, the differences between collarette, cactus, peony-flowered, and pompon types are so great that they might be placed in different genera if they occurred in nature. But they lack genetical continuity; every one of them must be propagated as a clon. Lacking this continuity, they are not recognized as kinds by the taxonomist. In the eastern states, as far west as Indiana, Phlox divaricata has rounded petals; farther west it has notched petals. Since the races are geographically separate, cross-breeding is impossible in nature. Obviously they have genetical continuity. Still the taxonomist keeps both types in the same species because the differences between them are trivial according to his standards. To be sure, they have been given varietal status, but both are still in the same species; they are all Phlox divaricata.

From all the evidence at hand, it seems that the taxonomist uses just two criteria in distinguishing kinds of plants: the one, a degree of morphological similarity within the population and of dissimilarity from other populations which is satisfactory to him; the other, a belief, seldom substantiated by experimental evidence, that each kind forms

a genetic continuum. Both of these criteria are essential; neither is sufficient without the other.

Turning now in the other direction from a kind of plant, we can envision a long series of assemblages of kinds, each of them in turn more inclusive in its scope. Every one of them is a population according to our definition of the term, yet they are rarely thought of as such. The fact that they are assemblages rather than kinds is attested by the names given to some of them, which are almost invariably plural. Every one of them is characterized by certain features of structural similarity, features not only distinguishable by the eye but subject to expression in words. Many of them are believed to be composed of genetically related plants. As examples, beginning with the Ragweed already used as such, we may cite such superior groups as Ambrosia, Helianthеae, Tubiflorae, Compositae (which is co-extensive with Asterales), Inferae, Sympetalae, Dicotyledoneae, Angiospermae, and Spermatophyta. But they are by no means all, since additional populations may be intercalated at many points in the series.

The taxonomist will at once see a vast differences between a kind of plant and these larger and more inclusive populations. He will call the latter classificatory groups and will regard them as actually or theoretically formed by successive syntheses. They are therefore easily divisible, in contrast to the kind or species. While they are considered to be related, he will regard them as theproducts of genetic discontinuity, that is, of evolution, while the kind is maintained by genetic continuity.

A kind or species of plants marks a turning point in our ideas. Above it we synthesize; below it we can but do not divide. The superior synthesis is always possible. Since it may be done in various ways, it has led to our manifold systems of classification. The inferior division is always possible but is not used. Why not? Because, by our definition, a kind (or species) of plant is the smallest population which has satisfactory morphological distinctness and inferential genetical continuity. If a kind is subdivided, the minor populations are separated by features which we regard as trivial, or they lack genetical continuity, or they fail in both essentials. The Ragweed has never been so divided. To be sure, the variety or form integrifolia has been noticed, but it is rejected by most botanists; even if it were accepted it would still be a part of Ambrosia trifida. Neither has Phlox divaricata been divided; the characters of its variety Laphami are considered trivial. If they were not so regarded, the variety would satisfy our definition of a species and would by this time have been named Phlox Laphami.

I have been trying to develop a picture of a long series of plant populations, each in turn more comprehensive in its scope, beginning with small groups of comparatively few individuals and ending with the whole plant kingdom. This we can all grasp, but I have also been trying to emphasize the idea that this long series is hinged, so to speak, somewhere near the middle at what we are pleased to call a kind or a species of plant; that below this turning point the populations are obtained by subdivision of the species, while above it they are regularly regarded as groups obtained by the combination of species.

The species is the starting point from which we begin our taxonomic work. From it as a vantage point, we can turn in two directions and see entirely different landscapes. In one view we see a long series of populations, progressively increasing in size and scope, many of them valuable to us in the formation or expression of our ideas on classification and phylogeny, many of them distinguished by names commonly used by taxonomists, all of them regularly regarded as groups of species. In the other direction we see (or we can see, if we are so disposed), but with an entirely different mental attitude, a similarly long series of populations, becoming successively smaller the farther we look and becoming also, in our opinion, successively less important, less worthy of attention by the busy taxonomist. And why are they less important? Simply because they lack one or both of the fundamental requirements of a kind of plant, morphological distinction and genetical continuity. We may sometimes suspect the accuracy and completeness of our knowledge and opinions. We may investigate these minor populations with the improved and comparatively new methods taught us by the morphologist, the cytologist, the geneticist, and even the phytogeographer and the physiologist, not in the hope, but on the chance of finding more important characters which might lead us to change our opinion. Usually we do not find them.

The Concept of Category

Starting from the species and considering the successively larger groups which may be formed by associating other species with it, we find that many of these groups are of such importance that names are given to them, as Hibiscus, Malvaceae, and Malvales. These groups differ in their size and scope, the latter term merely signifying the degree of morphological similarity which the group exhibits. This is a matter of classification, not of category.

We are also required to indicate the relative rank of these groups by referring them to certain categories and by

designating them by descriptive terms, as genus, family, and order, and by certain indicative endins, as -aceae and -alea. The sequence of these categories is fixed by botanical legislation. This is ordinarily a useful bit of taxonomic formality, but one which can not always be fully justified by taxonomic logic. We may illustrate this by the tropical seashore plant Batis maritima. It is the only species of the family Batidaceae and of the order Batidales. This apparently indicates that the nature and degree of the differences which separate this one species from all other kinds of plants are of the same nature and degree as those which usually separate orders from each other. If this is true, it might be useful to admit the order Batidales, but still superfluous to distinguish Batidaceae and Batis, both of which are exactly coextensive. It might even be argued that, in a discussion of classification, Batis maritima might be contrasted directly with Ranales, Fagales, and other orders, and its order and family eliminated.

The principle trouble with the enforced use of categories is that no attempt has been made to supplement legislation with advice or requirement about their nature. The scope of any category above the species is left completely to individual opinion and usage. In taxonomic research we develop and express our own opinions and often change the category to which a population is assigned. In taxonomic practice (not the same as research) we commonly accept the opinion of some qualified student and are guided by usage alone.

As a result, categories above the rank of genus have been subject to frequent change since the time of Linnaeus, and the general tendency has been to increase the number of families and orders. Some changes of category have been based on significant new knowledge, as the union of Lacistemaceae and Flacourtiaceae or the transfer of Nyssa from the Cornaceae of the Umbellales to an independent family within the Myrtales. Very few, possibly only one, the Degeneriaceae, are based on the actual discovery of previously unknown plants. Even this remarkable plant could have been assigned to another family by only a slight extension of definition.

Sometimes the changes have been based on erroneous information. There are still among us some who remember the observation of chalazogamy in Casuarina. Shortly thereafter an entirely new superior category appeared in print, the Chalazogamae, a subclass of dicotyledons contrasted with Porogamae. Its content was the single order Casuarinales, including the single family Casuarinaceae, including the single genus Casuarina, including a handful of species. No one knew whether Chalazogamy was universal in Casuarina; no one knew that it never occurred elsewhere. We know better today.

The vast majority of changes represent nothing more than differences of opinion. No new information is involved in assigning the pod-bearing plants of the Rosales to a single family Leguminosae or to three separate families, or the Oaks to one genus or to three. Their classification has bot been changed one iota.

Objections to changes of category at or above the level of the family are seldom voiced. To be sure, there was some mild protest in this country over the proposed division of Compositae into a dozen or more families. That was probably because the name Compositae was so familiar to all and not because of any intensive knowledge of the family. The great body of taxonomists work mostly at the level of the genus and species. Changes of category at the generic level are often severely criticized. Note the numerous criticims leveled at Rydberg, who advocated the division of Astragalus into a large number of smaller genera, or at Britton and Rose, who similarly divided Cassia. The odious word 'splitter' was often applied to all of them. A similar word was probably used on Nees a century ago, when he fragmented the old genus Laurus, although we of today are quite content to use Persea, Sassafras, Nectandra, and the numerous other segregate terms for these plants. One objection to Rydberg's action was that his segregations could not be maintained in the genus as a whole, including the Eurasian species, but I never heard this objection advanced by anyone who had personal knowledge of these foreign plants. The chief cause of all such criticism seems to rise from the fact that the generic name, under our binomial nomenclature, is part of the specific name, and any change of category at this level, whether by the division or the union of genera, is bound to result in change of name for some species. The whole purpose of our rules of nomenclature is to promote stability* of names.

Also below the rank of species certain categories are specified by name and must be used, if used at all, in a prescribed sequence. While these categories share with those above the species the complete lack of legislated definition, they differ radically in that they are not necessarily used. That difference is based absolutely on the nature of the species, which has already been discussed. Recognition of subspecific categories does not affect the species; every one of is still a part of the species and still bears the same name. It makes no difference whether we recognize Linnaea borealis

*Dr. Rogers correctly points out that the word in the International Rules is fixity, not stability. The general desire of botanists seems to be stability, while the original purpose of the Rules was apparently to promote uniformity.

borealis and L. borealis americana, or whether we call them subspecies or varieties; they are still all Linnaea borealis. The species is not divided. Since there is so little agreement about the connotation of categories below the species; since their epithets all have the form of a specific epithet; since their use is optional, not obligatory; since the differences between them are of minor importance or trivial; since they often have no genetical continuity, or are chance ephemeral mutants, or teratological forms, or mere ecads, the distinguishing of subspecific populations and their subordination in categories is a difficult and precarious undertaking.

The Concept of Classification

The only practicable way to investigate the classification of plants by the laity in early times is to search our language for group terms, but to consider only those which are purely names of plants and to exclude all which refer in any way to the properties or uses of plants. Oak and Maple are usable terms; Snakeroot is not.

English plant-names include hundreds of generic rank, each covering two or more species which resemble each other in structure and are more or less equivalent to modern taxonomic genera. Very often the species are named by adjectival epithets, resulting in binomials. Some popular genera are more or less co-extensive with taxonomic ones, as Oak and Elm; some were drawn too large, as Mint and Mustard, while others were certainly not recognized at all. For example, the English had the two words Leek and Garlic, to which were soon added from the French Onion, Chive, and Shallot, but our language does not have any one word for the genus Allium as a whole. The Romans knew five kinds of trees as Quercus, Robur, Cerris, Ilex, and Suber, but apparently had no generic word for Oak.

American usage gives us little evidence about classification, since so many English names were misapplied by early colonists to unrelated plants and many English plants and plant-names had been forgotten before their American relatives had been discovered. Ivy, as applied here to Rhus radicans and Kalmia latifolia and in England to Hedera Helix, Buckeye and Horse Chestnut do not indicate that they were regarded as two groups or genera of plants. Excellent examples of American generic names are Hickory and Goldenrod. A farmer may call Carya ovata Shellbark, but he will tell you that it is a kind of Hickory. On the other hand, in the few inquiries I have made, I have not found a farmer who uses the name Goldenrod for a member of the section Euthamia. The common people of Illinois half a century ago knew the genus

Sweet William, with its three species Phlox divaricata, P. pilosa, and P. glaberrima, but they did not know the equally conspicuous genus Silphium. Its two species were kept separate as Prairie Dock and Rosinweed.

Of superior classification into families we can find scarcely a trace, unless we regard the ancient terms Grass, Mustard, and Mint as of this category. In modern times Palm, Orchid, and perhaps others have been added to this list. Beech, Oak, and Chestnut were common words in England, yet our language contains no word for the Fagaceae. No families are more easily recognized than Umbelliferae and Leguminosae, but we have no English word for either. 'Pulse,' sometimes used by botanists as the English name of the latter family, properly refers only to the edible seeds of peas and beans.

The first faint trace of classification appears in numerous works of the sixteenth and seventeenth centuries. Then as now it was customary to divide books into chapters, and the herbalists often put into one chapter their discussions of plants having similar habits or similar uses. If this can be called classification, it is based mostly on the relation of plants to man. Not until the eighteenth century, with several earlier exceptions, were plants studied as objects of interest in themselves, and botany as a science was differentiated from botany as a part of horticulture and medicine. The number of known species grew rapidly; they were organized into genera.

There seems to be an innate urge in the human mind to keep knowledge in small and conveniently assorted packages, small enough that we may easily comprehend their contents, assorted so that we may easily compare one with the other. Even today we search the larger genera for ways to sort the species subgenera, sections, and species-groups. Following this urge and departing completely from the humanistic viewpoint, Linnaeus sorted his numerous genera according to his well known sexual system. There followed a full century in which taxonomists were busily engaged in discovering more genera and species and a few, notably Jussieu and DeCandolle, were attempting to classify these genera into superior groups. Every proposed change in classification was intended to set up groups based on a greater number of similarities or on structural features which their authors considered more important. They did not understand what the word 'important" meant.

The general acceptance or organic evolution explained the meaning of the term and gave an impetus to fresh attempts at improvement of classification. For the past century all revisions of classification have been made in the hope of a better expression of the course of evolution.

Conclusions

The task of the taxonomist is and has been the discovery of the kinds of plnats which exist, the description of them so that they can be recognized by others, the assignment of convenient names to them, and their classification in accordance with their probable evolution. Great progress has been made; much remains to be done.

In the United States and many other countries, taxonomists have the advantage of easy travel and huge collections of preserved material and consequent extensive acquaintance with plants. They are not discovering many 'new' species; their chief business is reforming their odeas of thoee already known. It is evident that the vast majority of thinking taxonomists, knowingly or unwittingly, base their species on the two features of morphological distinctness and genetical continuity which have been emphasized so often in this paper.

Nevertheless, we are frequently criticized for lack of agreement among ourselves. We admit that there is such disagreement at times, but we can easily find the causes of it and see that our work has constantly tended and doubtless will continue to converge on general agreement.

Certainly more than half of the criticism has been based on the use of different names. In many instances this is solely a matter of nomenclature, not of taxonomy, and with the general use of the International Rules of Nomenclature such differences are rapidly disappearing. There are some features of the rules which are elusive and may be interpreted in different ways, as the validity of publication of some of Muhlenberg's names. The various articles and sections of the rules do not obviously conflict, yet there are isolated examples in which different names for the same plant may apparently be legal under different clauses. The rules still lack clearly expressed directives for typification and some difference in usage may persist until this is remedied. The rules fortunately do not require that we must guess at the application of names published originally with scanty or faulty description and not associated with an authentic type specimen. Too much of that has been done in the past.

Another cause for criticism of names has resulted from the segregation and union of genera. Usually we think of it as a result of segregation, but that is because there are more 'splitters' than 'lumpers' among us. Few American botanists will care if I segregate the tropical genus Miconia into a dozen fragments (I shall not do so), but we can imagine the howls that will rise to the stratosphere if the Pin Oak appears in a frequently consulted reference book as Erythrobalanus palustris. Whether we call it uniformity, stability,

or fixity of names, that is what the botanist wants and what he ought to have. After all, why are genera segregated? I know very well the reasons used by Small and Rydberg. Rydberg's segregates differed from each other in the same general way and the same general degree as other commonly accepted genera of the same family. Small's segregates differed in what we often call 'technical characters,' as distinguished from the vegetative characters which may separate species. For this reason he segregated Wallia from Juglans, the former having a very rough nut, and Oligoneuron with striate bracts from Solidago. We admit that such procedure will suggest to any tyro that the Butternut and Black Walnut are much alike, but it will obscure tha fact that both are much like the English Walnut. How many amateurs and non-taxonomic botanists are eager to get such information? And if they consult Small's Flora to find out, can they not get it just as easily through the use of appropriate subgenera or sections? Similar reasons can be adduced for the union of genera and families. Some are not separated by characters of generic or familiar significance, whatever that may mean, as Aster and Solidago, Rubiaceae and Caprifoliaceae. Some tend to merge and have to be separated more or less arbitrarily, as Anemone and Hepatica. Close relationships are concealed by the use of different generic names, as Houstonia and Hedyotis.

All arguments for segregation may be condensed into a single legitimate excuse: the proponent wants greater homogeneity within the group. All arguments for combination may be reduced to the converse: the author wants greater separation between groups. Both desires are purely matters of individual opinion. We believe that our system of classification represents fairly well the course and the end results of evolution. In the end results, which are the species existing today, we can distinguish populations of specific rank which resemble each other more that they resemble any other populations. These make up a superior group which deserves taxonomic attention; it may or may not deserve a name. If it is given a name, it then must be assigned to a category, and there is nothing in the entire history of taxonomy to guide us to the proper choice of category except individual opinion; nothing, that is, in the plants themselves. There is an independent guide, the innate desire of botanists to have their knowledge classified, as I said before, in packages of convenient size and to have a stable system of names. We should keep this desire in mind when we contemplate changing a category, but we must not let it deter us from expressing what we sincerely believe to be the course of evolution.

At the present time there is very little discrepancy among taxonomists in the recognition of species. The vast majority of species in the United States, as presented in current Manuals, are accepted by all authors in precisely the same circumscription. Errors have been made in thepast and corrected later, and other errors may still be extant which we shall discover and correct in the future. We have done well to achieve this happy condition.

There is greater discrepancy in the subdivision of species into minor groups. These may be regarded by us as the different manifestations of a species, not distinct enough to be called independent species, but conspicuous enough to draw the attention of the observant botanist. They are not homogeneous in nature. They may be caused by slight differences in genes, or by mutation, or by the direct effect of the environment; they may or may not be correlated with geographic distribution. There is no necessity of describing or naming and of them, and failure to give them names or to use names for them can not be regarded as a denial of their existence. Names should be given and used only when they serve some useful taxonomic purpose. Otherwise we may conceivably approach the chaor imagined in this paper for Ambrosia trifida, or the septinomials of Stellaria and Saxifraga, or the triplicate sets of varieties in Cruciferae.

There are still several unsolved problems in taxonomic procedure, most of them connected with the process of evolution. We all believe that evolution has occurred and has produced our present flora; it is sometimes hard to understand that it is still proceeding. The dictum of Linnaeus, that God made species, Linnaeus describes them, continued well into the present century and is still apparent in some taxonomic research. Under that view, we are competent to handle most of our plants but we do not know what to do with others. More and more apogamous plants are being discovered. Shall we call them species, as has been done ad nauseam in Hieracium, or varieties of original sexual species, waiting of course for the discovery of what that species was, or shall we invent a special new term to be used as their name, a new category to include them, and a new system of nomenclature for them? We are finding, with the help of the geneticist and cytologist, more and more hybrids, and hybrid swarms, and introgressions; more and more polyploids. Some of these anomalous populations seem to represent the inception of species, others the gradual merging of two or more species into one. At present we do not know how to refer such plants to a place in our legislated categories or how to give them appropriate names. Most of the confusion in taxonomy are caused by plants of this nature.

Your author is one of the so-called old-fashioned taxonomists, or, as they say at Harvard, a classical taxonomist. He is just modern enough to recognize these problems but sufficiently old-fashioned and conservative to offer no solution for them. So far as he is concerned, that is a task for the future. In performing it, the taxonomists must keep their heads and not be led astray by any clique of botanical new-dealers.

The settled results of taxonomic study, fortunately accepted by a great majority of taxonomists and for a great majority of American plants, seem to be based on a concept of the species essentially or quite identical with that presented in these pages. This concept implies a nature and degree of morphological similarity satisfactory to us under our standards, and almost all of us have the same standards.

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CHANGE OF NAME FOR CERTAIN PLANTS OF THE 'MANUAL RANGE.'

H. A. Gleason

During the preparation of manuscript for the forthcoming Illustrated Flora, it seemed desirable to change the status of a number of taxonomic groups. Most of these changes were reductions of species to varieties; for many of these varieties valid names were found and new names were unnecessary. For others the use of the valid varietal epithet under the proper specific term resulted in a new combination. Five apparently undescribed entities were detected and considered to be sufficiently important to deserve varietal status. Only three changes of specific epithets are included, two necessitated by the rules of nomenclature and one by the elevation of a variety to specific rank. All these changes and additions require publication in accordance with the International Rules of Nomenclature and these formalities are complied with below.

My concept of species and my opinion of infraspecific taxa have been presented in the preceding article. As stated there, varieties are regarded as manifestations of a species caused by slight differences due to heredity, mutation, or direct effect of the environment. There is no necessity of recognizing varieties or using varietal names, but failure to do so does not deny the existence of such minor populations. Names and descriptions should be provided only for those which are conspicuous enought to attract attention. All reductions of species to varieties have been made in accordance with my idea that a species must be clearly distinguished